3

March 2, 1871.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

In accordance with the Statutes, the names of the Candidates for election into the Society were read as follows:—

Andrew Leith Adams, Surgeon-Major.
Robert Dudley Baxter.
William Henry Besant, M.A.
Henry Bowman Brady.
William Budd, M.D.
George William Callender, F.R.C.S.
Edwin Kilwick Calver, Capt. R.N.
William Carruthers.
Frederick Le Gros Clark.
John Cleland, M.D.
Herbert Davies, M.D.
Walter Dickson, M.D.
Henry Dircks.
August Dupré, Ph.D.
Robert Etheridge.
Alexander Fleming, M.D.
Peter Le Neve Foster, M.A.
Wilson Fox, M.D.
Arthur Gamgee, M.D.
Thomas Minchin Goodeve, M.A.
Frederick Guthrie, B.A.
John Herschel, Capt. R.E.
Edmund Thomas Higgins, F.R.C.S.
Rev. Thomas Hincks, B.A.
Trevenan James Holland, Major, C.B.
Charles Horne.
Rev. A. Hume, LL.D.
Edmund Charles Johnson.
M. Kelburne King, M.D.
John Leckenby.
Alexander Moncrieff, Capt., M.A.
Thomas George Montgomerie, Major R.E.
Richard Norris, M.D.
Edward Latham Ormerod, M.D.
Oliver Pemberton.
John Arthur Phillips.
Richard Quain, M.D.
Edward James Reed, C.B.
George West Royston-Pigott, M.D.
Carl Schorlemmer.
John Shortt, M.D.
Peter Squire.
Edward Thomas.
Edward Burnet Tylor.
Cromwell Fleetwood Varley.
Arthur Viscount Walden, P.Z.S.
A. T. Houghton Waters, M.D.
Charles William Wilson, Capt. R.E.
John Wood, F.R.C.S.
Edward Perceval Wright, M.D.

I. "Further Experiments on the effect of Diet and Exercise on the Elimination of Nitrogen." By E. A. Parkes, M.D., F.R.S. Received January 28, 1871.

In the Proceedings of the Royal Society (No. 89, 1867, and No. 94, 1867) some experiments were published on the elimination of nitrogen, during exercise and rest, on a nitrogenous and a non-nitrogenous diet. The result of both series was so far to confirm the experiments which show that the changes in the nitrogen of the urine and fæces are small in extent and afford no measure of the work; but there did appear to be a slight effect produced in two ways:—

1. There was an increased, though slight, outflow of nitrogen after work.

2. There was apparently a slight lessening of the outflow during work, not dependent on diminution in the amount of urinary water. In the state of rest also, when the diet was equal, there was no lessening, but a slight excess, in the excretion of nitrogen as compared with a period both of forced and ordinary exercise.

Professor Karl Voit, of Munich, who has a worldwide reputation for his numerous and important contributions to this subject, and who denies that exercise produces any change in the nitrogen, has taken exception to some of these experiments, on the ground more particularly that the daily ingress of nitrogen could not have been kept sufficiently stable. I believe the experiments, showing as they do in two men a remarkable agreement in the amount of nitrogen eliminated, and the fact that the two great articles of diet by which nitrogen enters (meat and bread) were selected and weighed with great care* and from the analyses appeared to be of constant composition, prove that the alterations in the daily inflow of nitrogen must have been very small and less than those of the outflow.

Undoubtedly, however, to insure an absolute uniformity in the entrance of nitrogen in men, is a very difficult matter as long as only ordinary diet is given. The employment of prepared or concentrated food, on the other hand, cannot be considered as good as common diet for such experiments; for the body is unaccustomed to the particular form in which the food is given.

It was determined to repeat the experiments in two or three ways. First, not only to use ordinary diet with the usual attention to keep it as uniform as possible from day to day, but to select hours for rest and exercise when the influence of diet is least perceptible, viz. twelve or fourteen hours after food; then in a second series to use prepared food in which the amount of nitrogen is absolutely constant; and thirdly to use a diet without nitrogen.

Unfortunately the experiments on preserved food failed on account of the health breaking down in a few days and before any exercise could be taken. On the ordinary diet also an unexpected difficulty arose, but still the results are worthy of record. The experiments on the non-nitrogenous diet are confirmatory of the former results, as far as the increased elimination after exercise is concerned.

As the details of the experiments would occupy too much space, I have given only mean numbers when these were sufficient to fairly show the results, and have omitted all details of the chloride of sodium, free acidity of the urine, and other matters.

The subject of the experiments was T. C., a perfectly healthy soldier

* The meat was beefsteak, and was selected free from visible fat, and was always cooked in the same way, *i. e.* fried. The bread was the hospital bread, made daily with the same flour, water, and salt, baked at the same heat and for the same time, and having the same amount of crust and crumb.

who had never had a day's illness in his life. He was 25 years of age, weighed usually 145 lbs., and is of very temperate habits. He had been an iron-worker before enlistment, and is an extremely powerful man; the girth of the chest was $37\frac{3}{4}$ inches.

First Series of Experiments.

Ordinary regulated diet.

During 20 days the man received daily beefsteak weighing 14 ounces when raw, 1 ounce of fat for cooking, 16 ounces of bread, 1 ounce of butter, 6 ounces of milk, 16 ounces of potatoes, $1\frac{1}{2}$ ounce of sugar, 36 fluid ounces of infusions of tea and coffee, and 16 ounces of water. The amount of nitrogen was determined at 300 grains; it might be a little more or less, but still from day to day its amount was the same as far as it could possibly be kept so. This diet was selected in this, as in the former experiments, because it is the usual ration of the Army Hospital Corps to which this man belonged, and therefore there was no fear of a change in the food itself producing any effect.

He took his meals always at the same time; viz. breakfast at 10 A.M., dinner at 3 P.M. (when he took the whole of his meat), and tea at $\frac{1}{4}$ to 6. After tea he took 6 ounces of water at 10 P.M., but no solid food.

The urine was collected from 10 A.M. to 6 A.M. on the following morning; then from 6 A.M. to 8 A.M. and from 8 A.M. to 10 A.M. As all food was taken between 10 A.M. and 6 P.M., it was expected that the urine from 6 A.M. to 10 A.M. (viz. from the 13th to the 16th hours after food) would be of tolerably constant composition; at any rate there would be less chance of error from the effect of food. In this way, and by keeping as far as possible an equal daily diet, it was hoped to lessen or remove the chances of fallacy from varying ingress of nitrogen. An unexpected circumstance partly disconcerted this hope.

It was anticipated that the amount of urine passing in the two hours from 8 to 10 A.M. would be less than in the two hours from 6 to 8, as being further removed from the time when fluid was taken. But the result was otherwise; there was always more urine passed from 8 to 10 A.M. than in the previous two hours. When this was first noted, it was supposed that an error in collecting the urine had been made; but day after day the result was the same. It seemed to be owing to the influence of sleep and wakefulness. From 6 to 8 the man slept, but from 8 to 10 he was not only awake, but his mind was active, and he talked to two men who worked in the room where he slept; and though his body was kept as quiet as during the previous two hours, the mental condition seemed to cause an increased passage of urine; at least there seemed nothing else to account for the fact that on every day during ten days while he was still in bed, there was more urine passed from 8 to 10 than from 6 to 8 A.M., although no water had been taken except at 10 the night before. The result was

that the urea did not fall as was expected, though its percentage was lessened.

Liebig's mercuric nitrate solution (the chloride of sodium being got rid of and the usual correction for dilution being made) and Voit's plan for the determination of nitrogen by soda-lime were both used, so as to afford a control of the observations.

The daily weight of the body, the temperature of the axilla and rectum, the pulse, the weight of the stools, &c. were also determined. Almost all the experiments were repeated, and several were performed three and four times*.

During the first ten days he remained in bed from 10 P.M. to 10 A.M., taking during the day his ordinary exercise. During the second 10 days he went to bed at 10 P.M. as before, got up at 6 A.M., worked for two hours, and then went to bed again at 8 A.M. until 10 A.M. The work consisted in dragging a cart weighing 710 lb. 4 miles in two hours. Supposing the coefficient of traction to be the same as in walking, the amount of work calculated by Haughton's formula would be equal to about 100 tons lifted one foot. His weight at the commencement of the experiment was 146 lb. 2 oz.; it fell regularly during 10 days to 144 lb. 10 oz., viz. 1 lb. 8 oz. During the second 10 days it fell to 142 lb. 12 oz., a loss of 1 lb. 14 oz.

The following are the mean results in the first and second sections of 10 days.

Mean amount of urine, in cub. centims.

	6 A.M. to 8 A.M.	8 A.M. to 10 A.M.	10 A.M. to 6 A.M.	Total.
1st section of 10 days	71·65	100·4	1077·45	1249·5
2nd section of 10 days.........	75·6	85·8	1068·8	1229·4

There was a slight decrease in the urinary water in the two hours of rest following the two hours' exercise.

Mean amount in grammes of mercuric nitrate precipitate taken as urea.

	6 to 8.	8 to 10.	10 to 6.	Total.
1st section of 10 days, usual exercise†	3·056	2·902	35·189	41·147
2nd section of 10 days, 2 hours additional exercise from 6 to 8	3·142	2·974	36·017	42·133

* Count Wollowicz commenced these experiments with me, but was obliged to discontinue them on account of the illness which eventually proved fatal. Serjeant Turner, of the Army Hospital Corps, very carefully took most of the observations on the temperature and the pulse.

† One day's urea from 6 to 10 A.M. is omitted as the urine was lost.

The results show no diminution in the urea during the two hours of exercise; the slight increase may perhaps be disregarded. In the two hours of rest, there is equally inconsiderable increase after exercise; the excess in the 4 hours of the second section of 10 days was 1·58 gramme. In the next 20 hours there was an increase of ·828 gramme, equal to ·3864 gramme, or 6 grains of nitrogen. The differences are so small as probably to fall within the limit of error, and it is impossible to affirm that exercise to the amount of 100-foot tons in two hours made any alteration in the urea.

Mean amount of nitrogen as determined by soda-lime.

	6 to 8.	8 to 10.	10 to 6.	Total.
1st section of 10 days*: rest from 6 to 10 A.M.	1·489	1·492	16·902	19·883
2nd section of 10 days: exercise from 6 to 8 A.M., rest from 8 to 10 A.M.	1·438	1·399	16·924	19·761

The nitrogen by soda-lime showed very slight variations. There was a slight decrease in the 2 hours' exercise and in the 2 hours' rest after exercise over the corresponding period; but it is so trifling that I hesitate to draw any conclusions. The figures show that the slight increase in the urea was, as supposed, an unavoidable error. Looking to the figures of the nitrogen by soda-lime as more correct, it seems that 2 hours' additional exercise produced no marked change in the outflow when the inflow of nitrogen was constant. This result, as far as it goes, certainly bears out Voit's assertion of the constancy of the nitrogen, but it does not destroy the conclusions formerly drawn that the nitrogen lessens during exercise and increases afterwards, because the amount of exercise was in the present case very much less; and as the alteration in the nitrogen even in the former experiments, with much more severe exercise, fell within narrow limits, it might easily have been anticipated that work of only one-sixth the amount would be inappreciable. The method of experimenting also does not appear to me to be so good as that formerly used.

Amount of nitrogen, in grammes, in the stools.

	Percentage composition.			Amount in 24 hours.		
	Solids.	Water.	Nitrogen.	Solids.	Water.	Nitrogen.
8th day of 1st period..	21·592	78·048	1·166	42·929	152·691	2·279
7th day of 2nd period.	25·00	75·00	1·379	26·115	78·345	1·441

* The nitrogen in the 2 first days is not included in the mean of the hours 6 to 10, as the urine was lost on one occasion, and on the other the experiment was uncertain. The mean of these hours (6 to 10) is therefore for 8 days.

On the 8th day there was a large stool, showing previous accumulation. Taking the weight of all the stools during the two sections of 10 days, and using the percentage composition of nitrogen of the one day in each series, the mean daily excretion of nitrogen was 1·807 gramme in the 1st section, and 1·766 gramme in the 2nd section of 10 days.

The pulse and the temperature of the axilla were taken every two hours from 6 A.M. to 10 P.M. The temperature is in Fahr. degrees:—

Mean pulse and temperature.

First section of 10 days.

	Hours.									Mean of day
	6.	8.	10.	12.	2.	4.	6.	8.	10.	
Pulse	64·6	66·3	67·8	79·3	65·5	75	74·6	74·2	69·5	70·65
Temp. of axilla	97·94	98·04	98·32	98·62	98·32	98·48	98·49	98·52	98·41	98·32
Temp. of rectum	......	99·1	......	99·2	......	99·27	......	99·48	......	99·26

Second section of 10 days.

(Additional exercise from 6 to 8 o'clock.)

	Hours.									Mean of day.
	6.	8.	10.	12.	2.	4.	6.	8.	10.	
Pulse	62·4	82·2	68·4	76	68	74·4	69	69·9	64·6	70·42
Temp. of axilla	98·24	98·24	98·33	98·23	98·41	98·45	98·42	98·44	98·38	98·34
Temp. of rectum	......	99·4	......	99·2	......	99·22	......	99·22	......	99·26

The effect on the pulse of the exercise from 6 to 8 in the 2nd period was interesting. The exercise brought up the pulse 16 beats at 8 o'clock over the corresponding period; but the pulse afterwards fell below the beats of the 1st period, and the result was a perfect balance of the day's work, so that the mean pulse of the day was the same in both periods. This shows how completely the heart, if called on for exertion, compensates itself by subsequent rest. The exercise made little difference in the temperature of axilla or rectum; there was a slight rise at 8 A.M. corresponding with the pulse after exercise in both axilla and rectum, but the mean of the day was the same.

Second Series.

Prepared concentrated food.

It was proposed to give prepared food of uniform composition, so that the daily ingress of nitrogen would be absolutely constant. I was unable to obtain the "pea-sausage" which the German troops are now using in the Field, and used instead a concentrated food which had been sent by an inventor to Sir Galbraith Logan, and by him sent to Netley for report.

It was composed of bread, meat, potatoes, sugar, spices and salt dried together, &c., and was stated to contain everything necessary for nutrition, so that the troops on service would need no other food. It contained 13·65 per cent. of water and 2·73035 per cent. of nitrogen. After preliminary trials to know how much would satisfy hunger, 14 ounces were given daily, containing 10·838 grammes of nitrogen. The food, however, produced such derangement in nutrition (indigestion, heartburn, and headache) that after a few days the experiments were discontinued. In spite of the constant ingress, the elimination of nitrogen varied greatly from day to day, the extreme range being from 7·641 to 15·024 grammes; and the man felt so ill that he begged to discontinue the experiment. It was interesting to note that, in spite of the daily variations in the nitrogen, there passed out in the 5 days nearly the same quantity as entered, viz. 54·920 grammes of exit, as against 54·19 grammes of entrance. He lost weight during the trial. The experiment must be therefore repeated on some future occasion with other prepared food.

Third Series.

Non-nitrogenous food.

In the experiments formerly related in the 'Proceedings' two men were kept for 2 days at a time without nitrogen. As it seemed to do no harm, the present experiments were now prolonged over 5 days on two occasions. The first was after the man had been well fed with nitrogen, the second after the body had become poor in nitrogen from the restricted supply given in the concentrated food. The non-nitrogenous food consisted of arrowroot, butter (deprived of casein), and lump sugar. Infusion of tea without milk was allowed, but this contained in the day only $\frac{1}{2}$ grain of nitrogen. Hunger was perfectly satisfied by this food; the man felt quite well and could have continued it. The heartburn produced by the concentrated food was at once relieved by this starch and fat diet.

First Experiments on Non-nitrogenous Food.

Previous daily entry of nitrogen = 19.5 grammes.

On the first day of non-nitrogenous food he took his ordinary exercise; on the 2nd took additional exercise, which consisted in digging up potatoes over 576 square feet, lifting the weight (16 stone) into a barrow, and wheeling them home for $\frac{1}{2}$ a mile. On the 3rd day he rested, on the 4th repeated the exercise, on the 5th rested. He did the 4th day's work even better than the 2nd, and could have worked on the 5th day.

The amount of work done cannot easily be calculated; it was a good but not an excessive day's work.

The weight on the first day was 142 lb. 7 oz., and on the last 141 lb. 10 oz. He took daily 60 fluid ounces of water (= 1704 cub. centims.), and as much arrowroot, oil of butter, and sugar as he liked.

Urinary water, in cub. centims.

	8 A.M. to 8 P.M.	8 P.M. to 8 A.M.	Total.
1st day, usual work	676·5	430	1106·5
2nd day, exercise	660	270	930
3rd day, rest	780	210	990
4th day, exercise	415	87	502
5th day, rest	715	140	850

Urea, in grammes.

	8 A.M. to 8 P.M.	8 P.M. to 8 A.M.	Total.
1st day, usual work	16·768	8·514	25·282
2nd day, exercise	7·986	4·293	12·079
3rd day, rest	3·042	3·087	6·123
4th day, exercise	3·652	2·540	6·192
5th day, rest	4·290	3·066	7·356

Nitrogen.

	Nitrogen by soda-lime.		Total in 24 hours.	
	8 A.M. to 8 P.M.	8 P.M. to 8 A.M.	Nitrogen by soda-lime.	Nitrogen calculated from urea.
1st day, usual work	8·287	4·304	12·591	11·798
2nd day, exercise	3·782	2·381	6·163	5·728
3rd day, rest	1·692	1·396	3·088	2·859
4th day, exercise	1·764	·9196	2·684	2·889
5th day, rest	2·052	1·333	3·383	3·433

The effects of exercise and rest are complicated with the gradually decreasing elimination dependent on the supply of nitrogen being cut off, consequently nothing can be concluded from the lessening of nitrogen on the 2nd day (exercise). On comparing the 3rd and 4th days (rest and exercise), the nitrogen by soda-lime shows a decrease on the exercise day; but as the ureal nitrogen is almost precisely the same on the two days, it does not seem possible to affirm a decrease. The most positive result is the increase of nitrogen (as shown by both methods) on the 5th day (rest after work). There was a decided excess, amounting to ·699 grammes or very nearly 19 per cent. Such an increase occurring on the 5th day after the supply of nitrogen was cut off, seems inexplicable unless on the supposition that it was owing to the previous muscular exertion.

I felt, however, that this experiment might be better conducted. The exercise was commenced too soon, and before the nitrogen had reached its

lowest point. The kind of exercise, too, viz. digging, which is often attended with intervals of rest, was not a good choice. Moreover, on the 3rd and 4th days he took some common salt, which might possibly have interfered, as it is supposed that chloride of sodium augments the outflow of urea.

The experiment was therefore repeated at the period when the body was poor in nitrogen from the use of the concentrated food.

Second Experiments on Non-nitrogenous Food (Arrowroot, Oil of Butter, and Sugar).

Previous daily entry of nitrogen = 10·838 grammes.

In this series the man did his ordinary work, which was not severe, and tolerably uniform, during the first 3 days. Then on the fourth day he marched 32 miles on level ground, carrying the new valise equipment, the service-kit, 40 rounds of ball ammunition, rifle, bayonet, and great coat. In all the weight was 43½ lb., his own weight being 145½ lb. The work done was therefore 712·8 tons lifted a foot, or, in other words, it was an extremely hard day's work. He did 26 miles quite easily, but then was greatly fatigued and got very tender about the feet, and had pains in the calves of the legs. The next day he was, however, quite well, and declared that he did not feel in the least weakened from having been 5 days on starch and butter. The march commenced at 8 A.M., and with intervals of 1½ hour for meals, lasted till 7.30, so that actual muscular work both of arms and legs was going on for 10 hours. The daily ingress of chloride of sodium was uniform during the 5 days.

Elimination of urinary water, in cub. centims.

	8 A.M. to 8 P.M.	8 P.M. to 8 A.M.	Total.
1st day, usual work	950	570	1520
2nd day, usual work.........	920	550	1470
3rd day, usual work.........	800	470	1270
4th day, marching	585	325	910
5th day, rest	765	495	1260

Urea, in grammes.

	8 A.M. to 8 P.M.	8 P.M. to 8 A.M.	Total.
1st day, usual work	....	....	13·072
2nd day, usual work.........	....	....	10·731
3rd day, usual work.........	....	....	9·271
4th day, marching	4·563	2·762	7·323
5th day, rest	9·562	6·435	15·997

Nitrogen by soda-lime.

	8 A.M. to 8 P.M.	8 P.M. to 8 A.M.	Total in 24 hours.	Total nitrogen calculated from urea.
1st day, usual work	....	....	5·936	6·100
2nd day, usual work	....	....	5·427	5·008
3rd day, usual work	....	....	4·328	4·327
4th day, marching	2·451	1·361	3·812	3·418
5th day, rest	4·997	3·268	8·265	7·465

The effect of the previous small entry of nitrogenous food is clearly seen; on the 1st day the nitrogen fell almost to the amount of the 2nd day in the previous experiments. On the 3rd day, on the contrary, it was greater than on the corresponding day of the former series.

The amount of nitrogen was actually greater on the 5th day than on the 1st. Except the excessive exercise of the 4th day, no other obvious cause existed for this elimination on the 5th day. No mistake seems possible; for the urinary water on the 5th day was less in quantity than on the 1st, 2nd, and 3rd days, while the nitrogen was 2 grammes more than even on the 1st day after nitrogenous food was left off. An error in analysis is not possible, since not only were the analyses repeated, but the process by urea gave results corresponding to that by soda-lime. No constitutional condition which could cause excess in elimination was indicated either by the pulse or body temperature, and the man felt perfectly well. I need hardly say that no nitrogenous food was taken; for it is quite certain that it was not.

The increase in the 5th day in the 1st series, though less marked, is still unequivocal, and there seems therefore no rashness in stating that the conclusion of the experiments formerly laid before the Society is affirmed, viz. that severe exercise causes an increase in the elimination of nitrogen in the period of rest after the exercise. It is noticeable that in this man the increased elimination was not in the hours immediately succeeding, but on the following day, and lasted for some time.

Whether during the period of exercise the nitrogen was lessened is not so clear, as the fall from 4·328 grammes on the 3rd day to 3·812 on the 4th or exercise day might be merely the continuing effect of the deprivation of nitrogen. The experiments formerly recorded seem to me better adapted to determine this point, which, however, certainly requires more evidence in confirmation before it can be accepted.

That changes go on in the muscles during exercise which lead to an increase in the outflow of nitrogen afterwards must, I think, be admitted; and on this point it seems that the statement of Liebig must be supported against Voit.

It may be interesting to give the mean pulse and temperature during these days of non-nitrogenous feeding for comparison with the normal.

During the day of exercise, however, the observations at 10 A.M. and 2 P.M. on the pulse and all the temperature observations on the marching day were lost, except at 8 A.M. and 8 P.M.

On a diet without nitrogen.

Mean pulse.

	Hours.							Mean of day.
	8 A.M.	10 A.M.	12 NOON.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	
1st 5 days ...	64·4	69·6	69·2	73·6	70·2	75·8	72·6	70·77
2nd 5 days...	68	70·25	75·2	75	74	75·6	72·6	72·95

Mean temperature of axilla.

	Hours.								Mean by day.
	6 A.M.	8 A.M.	10 A.M.	12 NOON.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	
1st 5 days ...	98·08	98·16	98·36	98·36	98·32	98·32	98·36	98·36	98·29
2nd 5 days...	......	98·08	98·15	98·25	98·2	98·15	98·5	98·15	98·16

Mean temperature of rectum.

	Hours.							Mean of day.
	8 A.M.	10 A.M.	12 NOON.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	
1st 5 days ...	99·0	......	99·2	......	99·4	......	99·4	99·25
2nd 5 days...	98·7	......	99·04	......	99·24	......	99·48	99·115

The mean pulse on the 5th day of the 1st series of non-nitrogenous food was 69·3, and 67·9 on the 5th day of the 2nd series. Both these were rest days, when the heart would naturally beat rather less. Non-nitrogenous diet does not, therefore, materially affect the number of beats of the pulse; but it decidedly influenced its volume, rendering it smaller, far softer, and more compressible when felt with the finger, and it gave a feeble sphygmographic tracing.

The sphygmographic tracings show this clearly. Ten tracings were taken at 6 A.M. on successive days, when the man was on nitrogenous food, 15 hours after dinner and 12 after the last food. They represent, therefore, the tracings of inanition. Five tracings were taken on successive days at 6 A.M. on the non-nitrogenous food. The same instrument and an equal pressure were always used. The height to which the lever was raised (all conditions of pressure &c. being equal) will therefore show the expansion of the artery. The tracings were carefully measured, and the following is the result:—

	Height of upright line in the sphygmographic tracing at 6 A.M., in inches.
1st day, nitrogenous food, pulse 12 hours after food	0·1375
2nd " "	0·1625
3rd " "	0·1437
4th " "	0·2000
5th " "	0·1262
6th " "	0·1187
7th " "	0·1500
8th " "	0·1125
9th " "	0·2125
10th " "	0·1375
Mean...............	0·1501

1st day, non-nitrogenous food	0·0750
2nd " "	0·0810
3rd " "	0·1250
4th " "	0·0761
5th " "	0·0625
Mean...............	0·0839

With an equal pressure the lever was thrown almost double the height when the man was on nitrogenous food. This feebleness of expansion shown by the sphygmograph was quite in accordance with the impression given to the finger. The softness of the pulse proved it was not owing to increased resistance of the arterial wall.

With regard to the temperature, the means are so close to those of the days on ordinary diet, that having regard to the fact that the period was shorter and therefore more liable to error, and that some observations were omitted on the marching-day, it may be concluded that a non-nitrogenous diet continued for 5 days neither raised nor lowered the temperature of the axilla and rectum.

It therefore appears that when the nitrogenous food of a healthy man was reduced to one half for 5 days, and he was then kept for 5 days more without nitrogen, he was able on the 4th day after such deprivation to do a very hard day's work. The non-nitrogenous diet, consisting of butter oil, starch, and sugar, kept him perfectly well; all functions seemed natural, the temperature of the body was unaltered, the pulse became very soft, and the sphygmographic tracings showed very feeble markings; but it was not materially altered in frequency. The circulation appeared to be properly carried on, as far as could be judged of by the man's own feelings. The health, as judged of by the man's feelings and the absence of objective signs, was perfect. On account, however, of the feebleness of the heart's

action it was not thought right to continue the experiments, which had, I believe, sufficiently proved that force necessary for great muscular work can be obtained by the muscles from fat and starch, though changes in the nitrogenous constituents of the muscles also go on which have as one effect an increased though not excessive elimination of nitrogen after the cessation of the work.

II. "Magnetic Observations made during a Voyage to the North of Europe and the Coasts of the Arctic Sea in the Summer of 1870." By Capt. Ivan Belavenetz, I.R.N., Director of the Imperial Magnetic Observatory, Cronstadt. In a Letter to Archibald Smith, M.A., LL.D., F.R.S. Communicated by Mr. Smith. Received February 4, 1871.

Dear Friend,—Last summer I made a very interesting magnetic voyage, being invited by Vice-Admiral Possiet to take part in the Arctic Expedition with the Grand Duke Alexis, Lieutenant of the Navy.

The first part of the voyage, from St. Petersburgh to Arkangeslsk (1179 miles), by rivers and lakes, I made in a little screw cutter, $27\frac{1}{2}$ feet long, $7\frac{1}{2}$ feet wide, and $2\frac{1}{2}$ feet deep, belonging to the corvette 'Variage;' the second part of the voyage (1716 miles) in the schooner 'Sextant.'

I visited the White Sea and the coasts of the Arctic Ocean; the end of the voyage was in the clipper 'Jemchug' (2461 miles), from Norway to Cronstadt.

On the way I made magnetic observations, the result of which I inclose in this letter. I will ask you to make them known to General Sabine and to the Royal Society.

The observations were made by a small compass which has the edge needle, and which is able to turn from one side to the other. Each observation, the data of which are given, was made in different directions of the instrument, turning the instrument on 120° in azimuth, by which the eccentric errors were taken off. In each direction the needle was turned on both sides for correcting the error of the magnetic direction in the needle. Four observations were made for each position of the needle. By this mode of observation the error does not exceed more than $\pm 1'$.

In the declination table are given the day and the hour of each observation in order to judge of the daily disturbances of magnetism. It would be very interesting to compare these observations with those made at the same time by photography at Kew, and thereby to deduce the magnetic disturbance due to the change of magnetic latitude.

The inclination was observed by a Kew Inclinometer belonging to the Compass Observatory, made in London in 1865, and examined by Mr. Balfour Stewart. No doubt it is the most useful instrument for this kind of observation.

The horizontal force was observed by "Captain Belavenetz's Instrument

No.	Place of Observation.	Declination.				
		Month and Day.	Hour of Observation.	Observed Declination.	Mean Declination.	Month and Day.
			h m h m	° ′	° ′	
1.	Shlisselburg (town). Lat. 59° 56′ 38″ N. Long. 31° 1′ 59″ E. from Greenwich.	....	....	....	....	31 May / 12 June
2.	Lodejnse Pole (town). Lat. 60° 44′ 11″·8 N. Long. 33° 33′ 8″ E.	3/15 June	5 0 to 5 30 p.m.	1 22·7 E.	1 22·7 E.	3/15 June
3.	Vitegra (town). Lat. 61° 0′ 23″·4 N. Long. 36° 27′ 1″·4 E.	7/19 June	8 30 to 9 0 a.m.	2 14·5 E.	2 15·1 E.	
		7/19 June	9 0 „ 9 30 „	2 13·4		7/19 June
		8/20 June	9 0 „ 9 30 „	2 19·9		
		8/20 June	9 30 „ 10 0 „	2 11·8		7/19 June
		7/19 June	6 30 „ 7 0 p.m.	2 14·8		
		7/19 June	7 0 „ 7 30 „	2 15·9 E.		
4.	Vologda (town). Lat. 59° 13′ 30″·9 N. Long. 39° 52′ 59″·7 E.	15/27 June	10 0 to 10 30 a.m.	3 17·6 E.	3 24·8 E.	
		„ „	10 30 „ 11 0 „	3 22·0		
		„ „	11 0 „ 11 30 „	3 24·4		
		„ „	11 30 „ 12 0 noon	3 23·4		14/26 June
		„ „	4 0 „ 4 30 p.m.	3 33·9		„ „
		„ „	4 30 „ 5 0 „	3 26·9		16/28 „
		16/23 „	7 0 „ 7 30 a.m.	3 25·6		
		„ „	8 0 „ 8 30 „	3 25·9		
		„ „	9 0 „ 10 0 „	3 24·5 E.		
5.	Veliky Oostug (town). Lat. 60° 45′ 45″·6 N. Long. 46° 17′ 59″·2 E.	19 June / 1 July	5 0 to 5 45 p.m.	6 55·8 E.	7 8·8 E.	
		21 June / 3 July	7 15 „ 8 0 a.m.	7 03·8		19 June / 1 July
		19 June / 1 July	6 0 „ 6 40 p.m.	7 17·0		„ „
		21 June / 3 July	8 0 „ 8 45 a.m.	7 17·7 E.		
6.	Pianda (village). Lat. 62° 56′ 11″ N. Long. 42° 35′ 8″·3 E.	24 June / 6 July	4 15 to 5 0 a.m.	4 57·2 E.	4 57·2 E.	24 June / 6 July

elements: we do not mend matters in the least, but we gratuitously add confusion to our ignorance, by dealing with hereditary facts on the plan of ordinary pedigrees—namely, from the *persons* of the parents to those of their offspring.

It will be observed that, owing to the clearer idea we have now obtained of the meaning of kinship and of the consecutive phases of the chain of life, the various causes of individual variation can be easily and surely sorted into their proper places. I will mention a few of them, merely as examples.

Previous to the segregation of the embryonic elements, if the structureless ones be diverse without any strongly preponderating element, it is impossible to foresee the character of the embryo, just as it is impossible to foresee the character of a handful chosen from an urn containing a mixed assemblage of variously coloured balls; but if they be not diverse, then the embryonic elements will be a true sample of the structureless ones, the conditions of purity of blood are fulfilled, and the offspring will resemble its parents.

We also see, in the process by which the embryonic elements are obtained, how the curious phenomenon may occur of inheritance occasionally skipping alternate generations. The more that has been removed from the structureless group for the supply of the embryonic (which, as we have seen, is a nearly sterile destination) the less remains for the "residue," too little, it may be, to assert itself by that, the only prolific, line of transmission. In the supposed case it would recuperate itself during the succeeding generation, where the elements in question will have remained wholly latent, owing to their insignificance in the structureless stage of that generation, which would be sufficient to secure any portion of it from selection for the embryonic form.

Again, it is in the process of selection of elements, both latent and patent, from the adult parents for the structureless stage of the next generation, where I suppose the curious and unknown conditions usually to occur through which a change in the habits of life, after the adult age has been reached, is apt to produce sterility. I may be permitted to remark, hypothetically, that this view appears to be corroborated by the fact that many grains of pollen or many spermatozoa are required to fertilize each ovum, because, as it would seem, each separate one does not contain a sufficiently complete representation of the primary elements to supply the needs of an individual life, and that it is only by the accumulation of several separate consignments (so to speak) of the representative elements that the necessary variety is ensured. I argue from this that there is a tendency to a large individual variation in the constituents of each grain of pollen, or spermatozoon, and, by analogy, that there is a similar though smaller tendency in each ovum; also that changes in the habits of life may increase this variation to a degree that involves sterility.

Lastly, it is often remarked (1) that the immediate offspring of different races or even varieties resemble their parents equally, but (2) that great diversities appear in the next and in succeeding generations. In which stage does the variability occur? It cannot be in the first (class representation) nor in the second (development), else (1) could not have been true; therefore it must be in the third stage. A white parent necessarily contributes white elements to the structureless stage of his offspring, and a black, black; but it does not in the least follow that the contributions from a true mulatto must be truly mulatto.

One result of this investigation is to show very clearly that large variation in individuals from their parents is not incompatible with the strict doctrine of heredity, but is a consequence of it wherever the breed is impure. I am desirous of applying these considerations to the intellectual and moral gifts of the human race, which is more mongrelized than that of any other domesticated animal. It has been thought by some that the fact of children frequently showing marked individual variation in ability from that of their parents is a proof that intellectual and moral gifts are not strictly transmitted by inheritance. My arguments lead to exactly the opposite result. I show that their great individual variation is a necessity under present conditions; and I maintain that results derived from large averages are all that can be required, and all we could expect to obtain, to prove that intellectual and moral gifts are as strictly matters of inheritance as any purely physical qualities.

III. "Further Experiments on the Effect of Alcohol and Exercise on the Elimination of Nitrogen and on the Pulse and Temperature of the Body." By E. A. Parkes, M.D., F.R.S. Received April 25, 1872.

In the 'Proceedings of the Royal Society' (xviii. p. 362, xix. p. 73) are some observations by the late Count Wollowicz and myself on the effect of alcohol, brandy, and claret on the elimination of nitrogen. As the experiments were on one man, I have taken an opportunity of repeating them on another person; and as the late observations of Dr. Austin Flint (junior) on a man who walked 317 miles in five days have appeared to some persons to run counter to the now generally accepted view that exercise produces either no change or only insignificant changes in the urea, I have combined experiments on exercise with those on alcohol. With respect, however, to Dr. Austin Flint's experiments, it would appear that while the egress of nitrogen was determined with the greatest accuracy, the amount taken in was for the most part merely estimated by reference to Payen's Tables, and therefore there is no certainty that the ingress was what it is assumed to have been. The food also was very